trical resistance, the number of B.A. units adopted as representing the resistance of a column of mercury 100 centimetres in length, one square millimetre in section, at a temperature of 0° C. be ·9540."

It follows from this that the legal ohm of the Paris Congress, which is defined as the resistance of such a column 106 cm. in length, contains 1·0112 B.A.U., while the B.A.U. contains ·9889 legal ohm. Thus resistances which are expressed in terms of B.A. units may be reduced to legal ohms by multiplying by ·9889, while makers and others who have a standard B.A. unit can construct a legal ohm by making a coil equal to 1·0112 times that unit.

A resolution of the nature of the above was rendered necessary by the fact that the legal ohm is defined in terms of the resistance of a column of mercury, while the resistances in use in England are B.A. units. The relation between the two has been determined by different observers with slightly different results. The Committee hope to secure uniformity in the resistances sold to the public as legal ohms, by stating the number they intend to adopt for the purpose of issuing standards.

R. T. GLAZEBROOK,
Secretary Electrical Standards Committee of the British Association

The Yard, the Metre, and the Old French Foot

THE ratio of the metre to the yard can scarcely be said to be known with certainty at the present day. The latest and most exhaustive investigation of this question is due to Prof. W. A. Rogers of Cambridge, Mass. The value which he assigns to it (pending a final and authoritative comparison of the "Mètre des Archives" with its chief representative, by the International Commission of Weights and Measures) is—

39·37027 : 36.

This ratio is very nearly identical with the much simpler one—

35 : 32,

the error of which is less than 1 in 8000. If we disregard this error, the conversion of yards into metres can be effected with the greatest ease by the following arithmetical process. Thus—

Given	35 yards or	39·37027 yards
Subtract 1/10th	− 3·5	− 3·937027
Add 1/7th	+ ·5	+ ·5624324
Results	32·0 metres	35·9956754 metres
	Error	− ·0043246

By a singular piece of good luck, this error, small as it is, may be entirely removed by one more easy approximation: for dividing the last quotient by 130 gives + ·00432263—where the outstanding disagreement is less than a unit in the last given figure. Hence, if we wish for a closer conversion than will be given by the terms 1, $-\frac{1}{10}$, $+\frac{1}{7}$ (of the last), we have only to add further, $+\frac{1}{130}$ (of the last) to obtain the utmost accuracy at present possible.

The converse operation—to convert metres into yards—is not quite so short and easy in the closer approximation. The following shows the approximate and the more exact conversion:—

	Ex. 1	Ex. 2
Given	32 metres	36 metres
Add 1/10th	+ 3·2	+ 3·6
Subtract 1/16th	− 0·2	− 0·225
,, 2/100ths ...	—	− ·0045
,, 1/20th	—	− ·000225
Results	35·0 yards	39·370275 yards

The fact is, almost any conversion may be performed in some such way, in three or four operations; and it was not for the sake of the rigorous ones that this note was written, so much as to bring into notice the very close and useful approximation represented by the ratio 32 : 35.

By way of applying the rules usefully, we may take as examples:—To find the equivalent of a kilometre in yards, and of a mile in metres. Thus—

	1000 metres	... ...	1760 yards
+	100	... ...	− 176
−	6·25	... ...	+ 25·14286
−	·125	... ...	+ ·19341
−	00625	... ...	—
Answers	1093·61875 yards	...	1609·3363 metres

Here we see that the approximate ratio 32 : 35 entails an error of only 0·131 yard (or less than 5 inches) in one kilometre; or of 0·193 (or less than 2) decim. in one mile, so converted.

The old French foot, the sixth part of the toise—the famous "Toise of Peru"—survives now practically only in the Prussian toise, so far as that is not superseded by the metre. Whatever may be its present range, the ratio of the old French foot to the present English foot is curious. I believe it may be expressed, within the limits of error of the relation so far as can now be known, by the fraction 389/365. That is to say, the excess of the former is 24/365 × the latter, the two components of which fraction are such as may easily be remembered when once the coincidence has been noted. J. HERSCHEL

Collingwood, July 25

Fireballs

IN reference to the phenomena of fireballs the following notes may be of interest. Last year, in July, I was residing on Naphill Common, Buckinghamshire. About midday, during my absence at Oxford, a violent thunderstorm broke over the district, and appeared to extend from Oxford to London. On returning I found that the house had been struck by lightning, apparently in two places. One chimney was knocked in through the roof, the debris partly filling up my room. The kitchen chimney had also been visited, the lightning breaking some of the brickwork of the hearth, and passing a person cooking at the fire; two or three others were in the house at the time, but no one was hurt. On carefully examining the marks left, I found that a door in a room adjoining the one above-mentioned had been split, and some iron knobs knocked off and broken, the screw nails being removed out of the wood, and a large hole several feet square made in the side of the house. From examination of the outside of the wall at the foot of the kitchen chimney, the bricks showed displacement opposite the marks inside at the hearth. I believe a tree was struck, and a water-trap or cesspool shifted out of position. Some men using a reaping machine in a neighbouring field stated that they knew the storm was coming by the fire playing about the blades of the machine. A boy who had been near at the time said that he saw a large ball of fire fall on the house, which it seemed to enter; it then reappeared, and passed into the meadow. I therefore think it likely that the damage done to the rooms and side of house was due to the electric development called a fireball.

Glasgow W. J. MILLAR

The Swallowing of one Snake by another

AS the author of the article "A Cannibal Snake" (NATURE, July 3, p. 216) wishes to know whether an instance similar to that recorded by him has ever before been brought to notice, I feel bound to publish an occurrence which I witnessed many years ago, and of which I have often told, without ever putting it into writing. During the summer of 1857 I lived in the environs of Washington; as an amusement, snakes were kept in cages. Sometimes, in the evening twilight, when toads and frogs appeared on the garden paths we used to feed the snakes with them. The usual habit of the snakes was to seize the toad wherever the jaws happened to strike, and to move them afterwards along the body of the victim, so as to begin the process of swallowing from the head. Once I threw a toad into a cage containing two of the common water-snakes of that region (*Nerodia sipedon*, if I recollect right, is the scientific name of the species). Both seized the toad at the same time; the one near the head began at once to swallow; the other put its jaws in motion as usual, in order to get at the head, but in doing this it reached the head of its comrade, and *began to swallow that, as well as the toad.* This went on for some time, until about three-quarters of the one snake were ingulfed within the other. Then the snakes separated again, the swallowed one coming out covered with slime, but apparently unhurt and as lively as ever. It lived a long time afterwards. The snakes were of about equal size, and, as far as I remember, from 2½ to 3 feet long. I suppose that it was the swallowing snake, and not the swallowed one, that kept the frog, but I do not think I ascertained the fact at the time. The whole performance lasted a few minutes only.

Heidelberg, Germany, July 27 C. R. OSTEN SACKEN

The Red Sunsets

I NOTE in NATURE of July 3 (p. 229) an abstract of a communication of M. Gay to the Paris Academy of Sciences, made

on June 23, in which he suggests a connection between the red sunsets and the frequent rains. During the latter half of the past winter the rains were incessant in the Atlantic States of America, and the writer suggested that they were due to the volcanic dust in the atmosphere, in a letter published in the *Philadelphia Public Ledger* of February 23. In a subsequent issue, March 8, he called attention to Dr. Aitken's researches. Subsequently Prof. Heilprin, of the Philadelphia Academy of Natural Sciences, offered a similar suggestion.

Philadelphia, July 16 CHAS. MORRIS

THE SALTNESS AND THE TEMPERATURE OF THE SEA[1]

PROFESSOR DITTMAR'S researches, an account of which forms Part I. of this volume, have finally proved that, so far as the most refined analysis can go, the mixture of salts dissolved in ocean water has attained a state of chemical equilibrium. But, although there is constancy of proportion between the various salts, the ratio of the total salts to the water varies considerably in different parts of the ocean.

The great evaporation in the dry tropical areas and the removal of water by freezing in the Polar seas tends to increase the salinity in these places, while in the tropical zones of continual rain and in the Polar fringes where the icebergs melt, there is constant dilution going on. The determination of the salinity at different places and depths is of great oceanographic importance, and the problem of finding the salinity has been attacked in various ways. The most simple and straightforward is to evaporate a weighed portion of the water to dryness and weigh the residue, but this cannot be done without chemical change taking place. The magnesium chloride present decomposes with the water into magnesia and hydrochloric acid, and all the carbonic acid of the carbonates is driven off. Gay-Lussac showed long ago how to avoid the error due to the dissociation of magnesium chloride, but no means have yet been suggested for taking account of the carbonates in a total salt determination. Direct weighing being thus found inexpedient, the next best method would appear to be to find the exact amount of any one element present, and by means of a table of complete analysis, taking advantage of the constancy of composition of ocean salts, to convert that into the salinity by multiplication with a constant factor. This is the method which Prof. Dittmar prefers, and for the purpose he estimates the chlorine or rather the total halogen by means of his refinement of Volhard's process. When the salinity of water has to be determined at sea, this delicate method cannot be conveniently employed, and it has been customary hitherto to measure the specific gravity of the water very carefully and afterwards to reduce the results to salinities. An attempt has been made with considerable success in the United States to substitute the determination of the refractive index of water for that of the density, and thence to deduce the salinity by a formula. This method is pre-eminently adapted for use at sea, but it appears not to possess the necessary delicacy.

The only method by which the specific gravity of a fluid can be ascertained on board ship is by means of hydrometers, and as the extreme values for sea-water are, according to Mr. Buchanan, 1·02780 and 1·02400, apparatus of great delicacy must be employed. A very delicate glass hydrometer was used on the *Challenger*, yet in spite of its fragility and the thousands of observations that Mr. Buchanan made with it in all weathers, he succeeded in carrying the one instrument which he had used during the entire voyage back to this country unbroken. His description of the hydrometer is as follows:—

"Preliminary calculations showed that convenient dimensions would be about 3 mm. for the diameter of the stem and about 150 c.c. for the volume of the body, and from 10 to 12 cm. for the length of the stem. The tube for the stem was selected with great care from a large assortment, and no want of uniformity in its outward shape could be detected with the callipers. The tube for the body of the instrument was also selected from a number, in order to secure such a diameter as would give the instrument a suitable length. In order to provide against accidents, I had four instruments made from the two lengths of tubing. The glass work of the instrument being finished—except that the top of the stem, instead of being sealed up, was slightly widened out into a funnel—the instrument was loaded with mercury, until the lower end of the stem was just immersed in distilled water of 16° C. A millimetre scale on paper was then fixed in the stem, and the calibration carried on by placing decigramme weights on the funnel-shaped top, and noting the consequent depression on the scale. The whole length of the scale was 10 cm., and this portion of the stem proved to be of perfectly uniform calibre. Several series of observations were made in order to determine accurately the volume of any length of the stem. . . . When this operation of calibration was finished, the end of the stem was carefully closed before the blowpipe."

The constants necessary for making a specific gravity observation were all determined with the utmost care. They included the exact weight *in vacuo* of the instrument, the volume of the body, the volume of each division of the stem, and the expansion of the whole instrument for a degree Centigrade. These data were fully tabulated; and in addition tables were made of the total weight when each of a set of brass weights was placed on a small table that could be slipped over the top of the stem. These weights were necessary, as, without them, the stem would require to be of great length in order to serve for waters of different density.

In making an observation Mr. Buchanan always kept the water sample in the laboratory for a night in order that it might have time to attain the temperature of the surrounding air. He then placed about 800 c.c. in a glass jar supported on a swinging table, and immersed the hydrometer in it after ascertaining its temperature exactly. To insure the greatest possible accuracy two readings were frequently made with different weights on the table, the results separately reduced, and the mean taken as the density. The density was calculated in every case by ascertaining the weight of the loaded hydrometer and dividing it by the immersed volume, which is calculated from the temperature and stem-reading.

Prof. Dittmar examined very particularly into the probable error in reading Buchanan's hydrometer, and after a long series of experiments, described in the chapter on Salinity in Part I., he came to the conclusion that the difference between the salinity as calculated by it and by his direct chlorine determinations (*i.e.* $\chi_1 - \chi$ where χ stands for the permilleage of chlorine) amounted to $-{\cdot}042 \pm \delta$, δ being a variable the chances of which being greater or less than ·06 are equal, and are 4 to 1 in favour of its being less than ·12. The value of χ is usually between 19 and 20.

At first Mr. Buchanan reduced his specific gravities to the temperature of 15°·56 C. by Hubbard's tables, but Prof. Dittmar, in the course of his investigation of "The specific gravity of water as a function of salinity, temperature, and pressure," succeeded in constructing a much better table in which the variation of the coefficient of expansion with the salinity of the water is taken account of, and all Mr. Buchanan's results published in this volume have been calculated by it. A very ingenious graphic method of comparing Hubbard's results with Dittmar's and converting one into the other is given in Plate I. of Part II.

[1] "The Physics and Chemistry of the Voyage of H.M.S. *Challenger*." Vol. i. Part ii. "Report on the Specific Gravity of Ocean Water." By J. Y. Buchanan, M.A., F.R.S.E. Part iii. "Report on the Deep-Sea Temperature Observations obtained by the Officers of H.M.S. *Challenger* during the Years 1873-76." (London: Longmans and Co., 1884.) See NATURE, July 24, p. 292.